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Original Articles.

COMPATIBILITY OF CONSERVATIVE AND AGGRESSIVE SURGERY.

ANNUAL ADDRESS BEFORE THE SOUTHERN SURGICAL AND GYNECOLOGICAL ASSOCIATION.

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THE circumspect philosophy of former days taught us, what man has done man may do. But the developments of more recent times say, whatever is practicable may be undertaken, without regard to precedents. Conservative and aggressive processes are combined in progressive surgery.

Conservatism in the use of all the appliances of surgery is not inconsistent with the application of the most energetic means of relief in structural disorders. A misapprehension exists with many of our profession as to the true sphere of progressive surgery, and it is my purpose on this occasion to make a distinction between rashness in the employment of operative measures and boldness in the use of surgical means of relief when clearly indicated. Real advances in surgical practice have not been the result of cutting and slashing without due consideration, but have accompanied painstaking investigation of the conditions requiring the knife, and caution in the performance of operations. As a preliminary to any surgical procedure of a radical nature, correct diagnosis is essential, but to accomplish a proper understanding of a deep seated disorder it is often requisite to make an exploratory operation of greater or less magnitude.

The information based upon such an exploratory measure serves as a guide to any further surgical procedure. A verification of any complication, of a serious nature, by an exploratory step, may form a contra-indication for the more radical operation which has been contemplated. A great variety of means have been employed under the expectation of elucidating the doubts surrounding very obscure cases, some of which are of a precarious nature, proving far more hazardous than the disease or the proposed radical measure. It is evident that exploratory processes of this order should be ruled out of our surgical resources, and the prime consideration for our safe guidance in the exploratory operations is to be sure that the existing disorders shall not be aggravated by such procedure, nor that other graver developments shall be induced as a consequence of it. It is best always in cases involving mooted points to avoid the appearance of evil. In keeping with this precaution against the abuse of exploratory measures, it is proper to exercise great discrimination in the use of antiseptics in surgery. A most salutary result may be obtained by em-

ploying very active germicides in a case of septile contamination, whereas the same application may not be suited to an operation upon tissues in their normal state. It is now recognized by bacteriologists that certain preparations tend to destroy pyogenic organisms, and when they exist the use of such antiseptic means is indicated. On the other hand it is becoming fully understood by clinical experience that a resort to these germicidal agents, when the pyogenic organisms are not present, operates unfavorably upon the healthy structure. The reaction in the practice of many surgeons of large practical experience, from observing the absolutely hurtful effects of the ordinary solutions of corrosive sublimate, when brought in contact with the absorbent surfaces of normal structures, is tending to a limitation of this poisonous substance to external use. These do not belong to the class of "superfluous laggards" who are characterized as opposed to the methods of asepsis and antisepsis, by the author of a recent work on surgery. But they are of that independent order of creation who are not bound down by any prescribed formulas and are bold enough to abandon a vicious practice which has brought trouble in its train to many victims of routine treatment.

It is well known to all surgeons, who have investigated thoroughly the merits of antisepsis, that there are many germicidal applications far safer than corrosive sublimate, which meet all the requirements in surgery, and the day is not far distant when the use of the solution of corrosive sublimate will be excluded from all operations upon incised healthy structures. There is a gradual movement of the pendulum of antisepsis towards asepsis, and it is found that sterilized water is the safest and best wash for surfaces not contaminated by previous septic developments in their tissues.

Antisepsis has a legitimate field of use in surgery, and it is to be regretted that the careless extravagance of the advocates of sublimate solutions should have given this process a black-eye by poisoning patients with them, who could have been treated successfully with other antiseptic agents.

Great detriment to antiseptic treatment has ensued from the indiscriminate application of solutions of corrosive sublimate, but judicious men are now more guarded in employing them in surgery.

Upon the general principle that no surgical operation of any magnitude should be undertaken without proper knowledge of the habits of life on the part of the patient, I may advert to a fact which is overlooked by many operators, in regard to the importance of continuing the use of any stimulant or narcotic to which the patient has long been accustomed. Within my experience the interruption even of the tobacco habit has been followed by troublesome nervous depression, after operations, and the resumption of the use of this narcotic has afforded complete and prompt relief. I recall an instance of laparotomy for the removal of an immense cystic tumor of the ovary in a lady sixty years of age who, of her own accord, stopped smoking the pipe after the operation. For a few days all seemed to be doing well with the case, but at the end of a week there was a most profound depression of spirits with vital prostration, which immediately disappeared upon resuming her pipe and the final result was entirely satisfactory.

We all know that a patient who has been addicted to the use of alcoholic drinks for a considerable period before submitting to any surgical operation, cannot have it suddenly abstracted without being subjected to great disturbance of the entire nervous system. If he does not reach the point of perturbation to produce delirium tremens, there will still be such derangement of all the functions as to interfere materially with proper nutrition, which may pave the way to serious complications in the after treatment of the case. It is, hence, essential under such circumstances, to take into account the previous habit in this respect, and continue to give certain quantities of the alcoholic stimulant at fixed periods to avert any troublesome consequences. An individual may fail to give such information as to habits of life in regard to the use of alcoholic stimulants unless specially interrogated and yet would not hesitate to state the fact upon inquiry for the guidance of the surgeon in his case.

The most important matter, however, for our investigation is in regard to the

opium habit in one subjected to a grave operation. I am convinced from a case which has recently been reported to me, that operators of experience and with a full knowledge of the use of morphine by a patient, do not always realize the great detriment resulting from the sudden withdrawal of this article after an important operation. All abnormal conditions resulting from any habit should be compensated in the after treatment. Observation of the changes resulting from inflammatory processes, should be accompanied by a study of those modifications impressed upon the tissues by impairment or undue activity of the nerve element, which enters into their composition. That many operators fail to take into account the nervous system, in their surgical pathology, shows a lack of due consideration of the surroundings of a patient. While the legitimate field of surgery is the proper use of means of relief for organic or structural disorders, there are prerequisites in the recognition of the conditions warranting an operation and in the preparation of the patient for undergoing it safely, which should characterize the highest type of the surgeon.

The distinction of external and internal treatment is held by most European authorities as the basis of recognition for the practice of surgery and medicine. But this line of distinction does not imply that all disorders of the inner structures are to be left for the treatment of the practitioners of medicine. Nor should all the pathological conditions not demanding operations be excluded from the domain of surgery A proper recognition of the scope of surgery and medicine is that of organic and functional disorders of the system.

With this limitation of the province of the physician as contra-distinguished from that of the surgeon, the work of the former should be confined to such measures as are calculated to correct the performance of the functions of the different organs of the body whether internal or external.

When organic changes ensue, whether demanding the use of medication or a resort to operative measures, the case comes then within the field of surgery. It presents a modification of structures, more or less pronounced, which alters the constituents so as to produce a departure from the normal state of the part involved, in its size, shape or density. An incision or a puncture may be requisite to diagnosticate such a condition, but if this change of structure could be clear y ascertained without any explanatory operation, the case would still fall within the domain of surgery ; with this distinction between functional and organic disorders for the practice of medicine and surgery, so soon as a change in the state of the case is recognized by the physician, it should be transferred to the charge of surgeon, and he will still have the disadvantage of combating the latter stages of organic disorders. It may be that a skillful operator is not qualified for the highest attainment in surgery, from lack of proper precaution in proceeding with an operation.

The aim of the surgeon should be a due comprehension of abnormality in the structures of the part involved, and his end be to afford relief with the least possible injury to the organ or member which is the seat of disorder. Conservative surgery may be destructive of certain parts for the purpose of saving the structures, and the properly qualified surgeon should consider maturely every phase of the case under examination, so as to assume the responsibility of lopping off the disease and prevent its extension to the sound tissues. He who fails to use the knife or the cautery, when a resort to either would stay the progress of disease, is not the exponent of conservative surgery, but on the contrary, aids and abets the work of destruction in the parts implicated, while he contributes ultimately to the death of the patient.

Ignorance and inexperience often lead to sad results in meddlesome surgery, when limbs are sacrificed or organs mutilated, to gratify the desire to figure as a bold operator on the part of a would-be surgeon.

In such cases no high-toned member of the profession should shield the culprit from the charge of malpractice or from the assessment of damages by a court of justice. While the allegation of malpractice, without sufficient cause, should never be encouraged by the better class of practitioners, it would be well to bring home to those who rush into the

surgical arena, unknowing and unknown, the consequences of their rashness. Of the two evils over-caution against doing harm is far preferable to meddlesome surgery, and yet we would not hold him guiltless who stands with folded arms and suffers a patient to die who might be saved by a timely operation.

There is a field in surgery for masterly inactivity, and non-interference is to be highly commended when by an operation a fatal result is precipitated. The want of a proper investigation of a case presenting grave complication may lead a physician to delay in calling a surgeon until the opportunity for relief has passed and his interference can avail nothing, so that he is justified in standing aloof.

There are numerous instances of the failure in surgical procedure from the late period of performing an operation, and it is greatly to be desired that a surgeon should be called in proper time by physicians having cases under their care which are likely to require surgical interference. If a consultation should be called and the surgeon found no indications for an operation, then of course the physician would be relieved of any future responsibility in proceeding with the treatment. But most probably their joint attendance would lead to the more satisfactory result in enabling them to determine upon the conditions developed in the progress of the case which might warrant an operation.

The dilatory spirit manifested by patients and those around them, as to resisting surgical means of relief in acute cases, has proved a barrier to the adoption of the expressed views of surgeons in favor of prompt action in many cases of great urgency. All practitioners of considerable experience have had occasion to regret that the opportunity for forestalling a grave malady has been lost by the indisposition to submit to a timely operation, and they led a further hope to end in disaster.

In this connection, however, it may be appropriate to consider the claims of a different case to our attention; when death is inevitable without an operation, and the knife affords the only chance, however slim that may be, for escape, it is a fair mode of dealing with this class of cases, for the surgeon to put himself in the place of the patient and determine what he would desire for himself under similar circumstances. Most of us, I think, in the possession of our faculties, would avail ourselves of the operation, and hope for the best result of it. Viewing matters from this standpoint, if those interested in the patient, manifest a desire to take the risk of which all are apprised, I think we are warranted in operating even should the probabilities be greatly against a successful result.

It is very true that untoward results, even when expected, tend to discredit surgery and to give an excuse to others for declining an operation at a certain stage when it might serve a good purpose. But considering the surroundings of the individual alone, we have certain death without an operation staring us in the face; and if the patient, after being advised of the situation, desires an operation, it should be performed.

But we have a serious question for settlement among ourselves as surgeons, and I am more especially concerned in the proper adjustment on this occasion of the differences in the surgical views of those who are equally entitled to think and to act in regard to surgical cases of great gravity.

In the course of consultation with other surgeons I have sometimes submitted a definite view in favor of immediate operation, proceeding with a confident expectation of warding off a fatal result, when the response of my colleagues has been such as to discourage any interference. On some of these occasions the unfavorable verdict has been accompanied with the statement, that if I thought proper to take the responsibility of operation, that they would render any assistance which might be desired, without, however, expecting any benefit. Of course I declined to rush into the breach under such untoward circumstances; and yet it has occurred to me that there ought to be some expression from the profession as to the line of duty for one who feels prompted to undertake the rescue of a patient from a most perilous situation, in which there is absolutely no hope without a surgical operation.

At the risk of being considered heterodoxical I would draw the attention of the profession to a consideration of the

propriety of immediate operation in that desperate class of cases which result from the crushing and mangling of limbs, attended with profound shock. It has been the custom of most surgeons to watch and wait while means are resorted to for restitution of the vital forces by stimulants. With our deficient knowledge of the exact etiological factor in this anomalous condition, I am inclined to the view that a continuous baleful influence is propagated to the nerve centers from the disintegration of the structures involved, and that this may be modified favorably by a clean incision through sound tissues above the point of injury very soon after such violence to the parts. It strikes me forcibly that surgical relief within fifteen or twenty minutes after an accident, involving the muscles, bones, nerves and blood vessels, cannot intensify the shock, and that many of the cases left to die without any operation might be rescued by a prompt amputation of the member. The A. C. E. mixture offers the most favorable conditions for an anesthetic and tends to lessen rather than increase the prostration, while the operation should be done with all possible dispatch.

It may prove the most conservative surgery to lop off structures whose vitality is completely destroyed and under the proposed state of the parts there can be no prospect of any restoration of nerve power or circulation to the tissues by delay in undertaking an operation. A proper appreciation of the participation in shock by the ganglionic nervous system should impress the surgeon with the great importance of arresting the morbific influence at the very earliest period practicable by removing the cause while energetic correctives are employed.

At this point it may be appropriate to advert to the precautions requisite in all grave operative procedures to avert a depressing effect; independent of previous violence

I have been convinced, by observation, that the use of moderate doses of quinine and strychnine during twenty-four hours, preceding any important surgical operation, seems to ward off the nervous prostration. It has also been my custom to administer an alcoholic stimulant with a hypodermic of morphine and atropia within a half hour preceding such an operation, and these preliminary measures have been attended with most satisfactory results. Prevention of shock is preferable to combating it by energetic means after a surgical operation.

As a fitting conclusion of this whole matter I would suggest that no surgical measure should be resorted to without looking into and correcting all underlying departures of the physical organization from the standard of health. It may not be possible to restore the normal condition of the secretions and excretions, yet means should be used for their correction, so far as may be practicable as a preliminary step for even the most simple operation; as the surgeon never can know in advance what complication may be developed in the course of treatment.

To prepare a patient for undergoing any capital operation, when the forces of the vital organism have been exhausted by long suffering, it is requisite to support the system by tonics and nutritious food for days or weeks prior to the surgical procedure. A neglect of this precaution is inexcusable, except in cases of urgency, when the delay would be likely to aggravate the malady.

The after treatment in surgical cases is in like manner of great importance to secure a good result, and this should include not only medication and food, but proper seclusion from exciting associations and due regard to the hygienic surroundings.

Some Notes on the Corneal Astigmatism in Two Hundred Eyes Measured with the Ophthalmometer of Javal, in Comparison with the Total Subjective Astigmatism after Complete Mydriasis.

By G. E. de SCHWEINITZ, M. D.

[Read October 26, 1892.]

THE ophthalmometer of Javal and Schiotz requires no introduction. Its advantages as an instrument of practical use are everywhere recognized, and even those who look upon its readings with misgivings are not inclined, I think, to disregard it entirely or set it aside in the lumber-room. True, it has sometimes suffered from unduly enthusiastic advocacy, but plenty of conservative es-

timates of its undoubted value are extant, and may be consulted for trustworthy information.

It will be interesting for a moment to give a brief *resume* of the rise of ophthalmometry with this instrument in the United States. To Dr. Swan M. Burnett, of Washington, belongs the credit of having been the first one in this country to advocate its use and demonstrate its practical value, in a paper entitled "Ophthalmometry with the Ophthalmometer of Javal and Schiotz, with an Account of a Case of Keratoconus," which appeared in 1885.[1] Two years later his treatise on astigmatism was published, containing a description of the instrument and of its value as a means of measuring the corneal astigmatism. One year later he recorded an exhaustive analysis of the refraction of more than five hundred healthy human corneæ examined with the ophthalmometer of Javal and Schiotz,[2] and in the discussion which followed this paper, Dr. Henry D. Noyes praised the ophthalmometer of Javal, stating that it was his habit to employ it in every case where it was necessary to inquire into the refraction of the eye. In his work on diseases of the eye there is an excellent description of the instrument and of the method of using it.

In the years which have followed, we find papers on the subject of ophthalmometry with this instrument by H. D. Speakman,[3] by Koller,[4] and an earnest advocacy of its employment,an advocacy which has never ceased, and has never been couched in words of an uncertain tone, by Roosa.[5] In November,1891,Dr. F. W. Ring[6] published a good description of the ophthalmometer and its uses, based upon personal conversations with Tscherning, Bull, of Paris, and Javal, and upon notes selected and compiled from the *Memoires d'Ophthalmometrie.* At the forty-second annual meeting of the American Medical Association held in Washington, May 1891, Dr. Burnett placed upon record further contributions to keratometry, reviewing his previous work and reiterating his favorable opinion of the ophthalmometer. In the present year articles upon the subject have been written by E. Swasey,[7] Roosa, Würdemann,[8] VanFleet,[9] and others; while Valk, in the new edition of his *Errors of Rafraction*, gives a good description of the ophthalmometer, and records a table of comparative examinations.

Some unfavorable comments have been published, together with a series of tables not unlike the one which I hope to present this evening, by Dr. J. H. Woodward,[10] but even in this paper, although objections are made to certain inaccuracies, according to the belief of this author, he frankly admits that the ophthalmometer is a useful adjunct and helps to point the way to the diagnosis of astigmatism, being especially useful when this refractive defect is of high degree.

The most recent communications upon the subject in this country are the papers of A. E. Davis,[11] containing, as it seems to me, the best practical directions for using the instrument accurately. I have not attempted to give references to the many foreign papers upon this subject; anyone interested will find quite a complete bibliography in Javal's *Memoires.*

Thus we see that from the publication of Dr. Burnett's paper, in 1885, up to the present time, numerous communications have appeared in this country, many of them strongly advocating the employment of this instrument.[12] Practically there has been no dissent from the views which Burnett advocated nearly

[1] Archives of Ophthalmology, vol. xiv.

[2] Transactions of the American Ophthalmological Society, 1888.

[3] Archives of Ophthalmology, 1890, vol. xix, and New York Medical Record, 1890, vol. xxxvii.

[4] Journal of the American Medical Association, 1890, xv. 380 to 383.

[5] Medical Record, April 19, 1890; New York Medical Journal, March 28, 1891; Medical Record, March 26, 1892.

[6] Ophthalmic Record, November, 1891.

[7] Boston Medical and Surgical Journal, March 10, 1892. cxxvi, pp. 232 to 236.

[8] Journal of the American Medical Association, September 3, 1892.

[9] New York Medical Journal, July 9, 1892.

[10] New York Medical Journal, July 16, 1892.

[11] New York Medical Journal, Sept. 10, 1892.

[12] It should be stated that Dr. Welland has made a study of what he believes to be the main defects of the ophthalmometer of Javal from a mathematical standpoint, and has published his research in the Medical News. 1892, lx. 626 to 629.

eight years ago, and it seems eminently proper that Javal should characterize him, which I am sure we are all glad to do, as the champion of ophthalmometry in America.

The cases which I present for your consideration this evening are taken from my private practice exactly in the order in which they presented themselves for examination. Each case, after the usual tests for visual acuity, the amplitude of accommodation, and the balance of the external ocular muscles, was examined with the ophthalmometer of Javal, and the reading carefully recorded. The eyes were then subjected to complete mydriasis, with atropine, hyoscyamine, or cumulative instillations of homatropine. In the cases under thirty years of age, hyoscyamine or atropine was employed; in those over this age, cumulative instillations of homatropine. In each instance a complete paralysis of the ciliary muscle was obtained, because I am one of those who believe that this can be done with a strong solution of homatropine when properly employed, and, as is well known, I have full support in this belief from such careful observers as Risley, Randall, and Jackson.

From these observations the tables were arranged, as will be seen, by placing first the case number, then the sex of the patient, followed by the age, the ophthalmometer reading, the glass selected under a mydriatic, the difference in the axes, and finally the difference in the amount of astigmatism, provided there was any difference in these respects. In recording the ophthalmometer reading, it might have been written, as Davis has suggested, for example: "Astigmatism with the rule 1 D axis 90°+, or axis 180°—." Again, "Astigmatism against the rule 180°+ or 90°—." In order to save space, however, the repetition has been omitted and the ophthalmometer reading is recorded, for example, thus: O.S. 3 D axis 180; glass selected after a mydriatic—2.75*c* axis 180, and not, as might have been done, following the directions above, O.S. 3 D axis 90+, or axis 180—, glass selected after mydriatic —2.75*c* axis 180.

Many of the degrees of astigmatism are weak, representing 0.25 D, 0.37 D, and 0.50 D, or, in other words, indicating that a reading was made in fractions of a dioptry, according to the amount of overlapping of the steps of the reflector. Now it is a difficult matter to assure one's self that the corneal images of the reflector are exactly in apposition, and there is no doubt that it is easy to commit an error of 0.25 or 0.37 D, when the first adjustment of the ophthalmometer is made. To a certain extent this depends upon the skill of the observer, but nevertheless introduces an element of doubt. Woodward, whose article has been referred to, writing concerning this point, says: "In the appended reports I wish to be understood. I do not pretend to accurately bisect the image of one-half of one step of the reflector; when the reading is, for example, 0.25 or 0.75 D, I wish to convey the idea that the overlapping of the reflector was, as nearly as I could estimate it, one-quarter or three-quarters of the step." This is, of course, a very fair statement, and applies in the present instance. I presume no one would pretend to be certain of estimating exactly one-quarter of the step. Still practice develops reasonable accuracy, just as it does in a cataract section where we are accustomed to estimate the width of a millimeter, or even of a fraction of a millimeter.

ANALYSIS OF THE TABLES.—There were 150 eyes in which the ophthalmometer recorded astigmatism according to the rule. In 134 of these eyes the axis of the cylinder found with the aid of mydriasis corresponded with that determined with the instrument. In 16 eyes there was a difference in this respect, the least difference being 5°, the greatest 75°. In the latter case the ophthalmometer recorded distinctly a slight astigmatism (0.25 D) according to the rule, but the patient accepted positively the same degree of astigmatism, but with the axis of the glass contrary to the rule.

In 35 eyes the amount of astigmatism found with the instrument exactly corresponded with that revealed by mydriasis, but in 115 eyes there was a difference in this respect: In 6 of them the mydriatic correction was slightly greater in amount than that found with the ophthalmometer—four times 0.12 D, and twice 0.25 D; in the remaining 109 eyes the

ophthalmometer reading gave a greater degree of astigmatism than the mydriatic measurement—twenty-five times 0.12 D, sixty-five times 0.25 D, four times 0.37 D, fourteen times 0.50 D, and once 0.75 D, or in other words, the average error was less than half a dioptry, being exactly 0.2613 D.

There were 21 eyes in which the ophthalmometer recorded astigmatism contrary to the rule. In 18 of these eyes the axis of the cylinder found with the aid of mydriasis corresponded with that determined by the instrument. In 3 eyes there was a difference in this respect, the least difference being 5° and the greatest 15°. The amount of astigmatism was identical in 9 eyes, but was different in 12 eyes. In 8 it was more than the ophthalmometer recorded with mydriasis, and in 4 less than the ophthalmometer recorded with the mydriasis. The average increase in the degree of astigmatism found with mydriasis over that recorded with the ophthalmometer was less than half a dioptry, or exactly 0.295 D.

There were 22 eyes in which the ophthalmometer recorded no astigmatism. In 10 of these eyes mydriasis also failed to reveal astigmatism, but in 12 an astigmatism was developed after the use of the drug, once with the rule, and eleven times contrary to the rule. The case with the rule was only 0.12 D, and for practical purposes may be omitted. In the remaining 11 cases the amount of astigmatism varied from 0.25 D to 0.62 D, with an average of about 0.384 D. To recapitulate these results we have :

A. There were 150 eyes with astigmatism according to the rule, with—

	Eyes.
1. Correspondence in axes in . . .	134
2. Difference in axes in	16
	150
1. Exact correspondence in amount of astigmatism	35
2. Difference in amount of astigmatism	115
	150
1. Astigmatism greater with mydriatic than with ophthalmometer in	6
2. Astigmatism greater with ophthalmometer than with mydriatic.	109
	115

If to the last total the 35 eyes in which there was exact correspondence in the amount of astigmatism be added, we have again a grand total of 150 eyes.

B. There were 21 eyes with astigmatism contrary to the rule, with—

	Eyes.
1. Correspondence in axes in . . .	18
2. Difference in axes in	3
	21
1. Exact correspondence in amount of astigmatism	9
2. Difference in amount of astigmatism	12
	21
1. Astigmatism greater with mydriatic than with ophthalmometer in	8
2. Astigmatism greater with opthalmometer than with mydriatic in	4
	12

If to the last total be added the 9 eyes in which there was exact correspondence in the amount of astigmatism, we have again a grand total of 21 eyes.

C. There were 22 eyes in which the ophthalmometer recorded no astigmatism, with—

	Eyes.
1. No astigmatism found with mydriasis in	10
2. Astigmatism found with mydriasis in	12
	22
1. Astigmatism according to the rule in (only 0.12 D)	1
2. Astigmatism contrary to the rule in (0.25 to 0.62 D, average 0.384 D)	11
	12

D. There were 5 eyes in which the ophthalmometer recorded astigmatism, but with—

	Eyes.
1. No Astigmatism with mydriasis in	5

In each of these eyes the astigmatism was according to the rule and the amount 0.5 D. Two eyes were practically blind, one from detachment of the retina and the other from congenital amblyopia, and hence these are of no importance in the result:

	Eyes.
1. Astigmatism according to the rule	150
2. Astigmatism contrary to the rule	21
3. Astigmatism absent according to ophthalmometer	22
4. Astigmatism present according to ophthalmometer but absent with mydriasis	5
5. Practically blind	2
	200

REMARKS.

1. THE AXIS OF THE CYLINDER.—In the 171 eyes in which it was possible to institute a comparison in the axes of the cylindrical glasses obtained by the two methods of measurement, there was exact correspondence in 152 eyes, or in 88.9 per cent., and failure in correspondence in 19, or 11.1 per cent.

Touching the 11 per cent. of failures it should be stated that so much depends upon perfect illumination, steadiness of the patient's eyes, and above all, upon an exact level of the two eyes, that it is more than probable that this percentage represents some inaccuracy of the observer rather than of the instrument. Davis,[1] in a recent article on this topic, quotes a capital instance illustrating the care which must be exercised in this respect. He examined a patient in Javal's clinic, failing, however, to take the precaution to see that the patient's eyes were exactly level, and found that she had an astigmatism at an axes 5° from the vertical. Javal then sat down to the instrument, sighted through the transverse slit, leveled the patient's eyes and measured the astigmatism, finding the same amount that Davis had discovered but with the axis vertical. The trial lenses confirmed his reading.

Moreover, it is a fact which has been pointed out a number of times by most careful observers, that the axis of the glass determined under the mydriatic sometimes fails to correspond with that accepted by the patient after the return of accommodation. Personally, I have had little or no difficulty in this respect, but would be inclined always to adopt the mydriatic determination of the axis, unless successive measurements with the ophthalmometer agreed in giving a different result from this, and then, after renewed mydriasis, I would expect to find a correspondent in the two methods; in other words, I would think my first mydriatic correction had been in error.

Finally, it should be remembered, that the question of error creeps into all methods of examination, and it seems to me not unlikely that any one of us, having determined the axis of a cylindrical glass in 171 eyes under a mydriatic, would, if he repeated these examinations with renewed mydriasis, find at least a dozen eyes in which he would be inclined to alter the axis of the glass 5° or 10° from the first determination, especially in the weaker grades of astigmatism. I am not unmindful of the occurrence of cases (one of which is recorded in the list, while several others could be quoted) in which there was a notable difference in the axes determined by the two methods; for example, the ophthalmometer giving distinctly astigmatism against the rule, and the patient accepting only, both with and without mydriasis, a cylinder with its axis according to the rule. These cases are probably explained by the presence of lenticular astigmatism, which the instrument is not capable of recognizing.

2. THE AMOUNT OF CORNEAL ASTIGMATISM.—In 171 eyes in which a comparison is made between the amount of corneal astigmatism determined by the instrument and the total astigmatism under a mydriatic, there was exact correspondence in 44 eyes, or 25.7 per cent., and failure in correspondence in 127 eyes, or 74.3 per cent.

In 109 eyes with astigmatism according to the rule, in which the astigmatism found with the instrument was greater than the amount found with mydriasis, the average increase was a little more than 0.25 D, being exactly 0.2613 D. In 12 eyes with astigmatism contrary to the rule, in which the astigmatism found with the instrument was less than the amount found with mydriasis, the average decrease was again a little more than 0.25 D, being exactly 0.295 D. This we know from Javal's teaching, is the usual result, and that in weak astigmatism according to the rule, the ophthalmometer is from 0.25 to 0.50 dioptries stronger than the subjective measurements with atropine. In astigmatism against the rule the ophthalmometer is too weak by from 0.25 to 0.75 D.[2]

Touching this same point, Burnett has formulated the following law, to which he thinks they are only occasional exceptions: "For the total subjective astigma-

[1] Loc. cit.

[2] See also description of the ophthalmometer by F. W. Ring, M. D., in the Ophthalmic Record, November, 1891.

tism subtract 0.5 D from the corneal astigmatism when it is according to the rule, and add 0.5 D if the corneal astigmatism is against the rule. Bull, of Paris, quoted by Burnett, holds to a similar law, using, however, 0.75 D instead of 0.5 D. This, Dr. Burnett considers too high, and I certainly agree with him, my own experience being that 0.5 D is quite sufficient to subtract from the corneal astigmatism when it is according to the rule. In fact, as I have already pointed out, in the majority of cases it seems to be somewhat less than this amount.

That there are exceptions to these rules Dr. Burnett admits, and I think everyone who works with the ophthalmometer will find them. In the figures presented this evening we see that in six eyes with astigmatism according to the rule, the total subjective astigmatism was greater than the corneal astigmatism and in four eyes with astigmatism contrary to the rule, the amount of astigmatism given by the ophthalmometer was greater than that found with a mydriatic. It should be stated in regard to these cases, however, that the difference was usually 0.12 D, and only twice 0.25 D. Dealing with such weak degrees of astigmatism, it is just as likely that the eye of the observer was at fault, as that the instrument made an inaccurate record.

Again, we have exceptions to the rule in the fact that there was an exact correspondence in the amount of astigmatism in 35 eyes with astigmatism according to the rule, and in 9 eyes with the astigmatism contrary to the rule. Here again, with 6 exceptions, the astigmatism was of low degree (0.25 or 0.50 D). In the 6 exceptions, however, it was more than 1 D. and ran as high as 3.50 D.

In 22 eyes the ophthalmometer failed to record astigmatism and in 10 of them no astigmatism was found with mydriasis, but in 12 of them a low degree of this refractive defect was demonstrated after the use of a mydriatic (0.12 to 0.62 D). In all of the cases in which this occurred, save one which for practical purposes, as has already been pointed out, may be omitted, the astigmatism was contrary to the rule; and this again agrees with the teaching that has previously been given, namely, when no astigmatism is found with the ophthalmometer, it is very probable that there is a real astigmatism against the rule from 0.25 to 0.50 D.

This analysis of the results obtained by the measurement of 200 eyes will, I think, suffice. The results have been given without bias, and inasmuch as they practically confirm those of other observers who have found the ophthalmometer exceedingly useful in the determination of corneal astigmatism they may be accepted as an additional indorsement of the many that are already on record as to the decided value of this instrument of precision.

Even if the ophthalmometer of Javal did nothing more than give us the axis of the principal meridian with a reasonable degree of certainty, which it undoubtedly does, it would be a very time-saving and valuable instrument in the office of the practical ophthalmologist. It has been stated that when the astigmatism exceeds 2 D, we may be certain that we are within 5 degrees of the true meridian or axis, and when the astigmatism is slight, there may be a possible error of 10 degrees; but it is equally true that with the model of 1889 the precision is greater, and, judging from my own experience, I would have a confidence in the axis of the glass attained by this method of examination equal to that which I have with subjective examination under the fullest mydriasis, and when I associate the two methods of examination, I am as certain of this point as it is possible to be.

But it is certainly fair to assume that, within reasonable limits and in the vast majority of cases, the ophthalmometer gives very exactly the degree of astigmatism, and that the combined observations of a number of observers permit the formulation of a rule, like other rules, is subject to exceptions, which tells us very nearly how much shall be subtracted from the ophthalmometer measurement when it is a case of astigmatism according to the rule, and how much shall be added to it when it is a case of astigmatism contrary to the rule. No one claims perfection for the instrument any more than he claims perfection for any other method of examining the eye, but when the opthalmometer is carefully used, when the light is properly regulated and evenly distributed, when the eyes are carefully levelled, and when the observer, having corrected his

own refractive defect, has acquired reasonable accuracy in the manipulation of the instrument, I certainly believe, and think these tables fairly show it that Dr. Burnett is correct in stating that this is one of the most important instruments of positive diagnosis which has been given to us since the invention of the ophthalmoscope.

I am aware that much stress has been laid upon the fact that it is not an absolutely accurate measurer of astigmatism, and that disturbing factors come in to play, *e. g.*, the watering of the eye of the patient, and, as Dr. Roosa aptly calls it, "the moving of the corea under the instrument." It is for this reason that the examinations must be constantly repeated before making the final record or before ordering the glass, just as we would repeat a mydriatic correction until we were satisfied that we had attained the highest degree of accuracy. It would be just as reasonable to condemn the instrument on this account as it would be to condemn cataract extraction because one does not always have a docile patient and does not always make a classical section.

It is not the purpose of the present paper to discuss a question which is full of interest, namely, how far the use of the ophthalmometer displaces mydriatics from their function in the correction of anomalies of refraction. It is needless for me in this a sembly, I think, to state that I am very far from believing that the sun of mydriasis has set, or, indeed, that it ever will set. I have elsewhere expressed myself very positively upon this subject, because I believe that the functions of mydriasis in connection with the correction of anomalies of refraction does not alone involve paralysis of the ciliary muscle, but exercises a very much wider and more important office. I am perfectly willing to state, however, that careful work with the opthalmometer will render the necessity of the use of prolonged mydriasis unnecessary in many cases, which, previous to a knowledge of this instrument or to any skill that may have been acquired in its use, would have been classed as examples suited to the employment of a mydriatic drug.

I cannot resist closing these remarks with a quotation from Dr. Burnett's first paper on "Ophthalmometry with the Ophthalmometer of Javal and Schiotz," partly because I feel a personal indebtedness to him in learning the use of this instrument, and partly because it gives in a nutshell what the instrument will and will not do, and hence what we may expect of it:

"The ophthalmometer does not give us the refractive condition of the eye as a whole, and furnishes no positive indication as to the existence of myopia or hypermetropia, but it gives with exactness the radius of curvature of the cornea in all its meridians; and where there is a difference, it shows the direction of the principal meridians, and we can read on the instrument the amount of the difference in dioptries and fractions. The inventors claim that a difference of 0.25 D can be readily detected, and my own experience would substantiate this statement. The main question is, Is it practical? I most unhesitatingly answer, Yes. Taking all things into consideration, it seems to me the most practical of all of the instruments of precision we use in the diagnosis of astigmatism."

A CLINICAL DESCRIPTION OF DYSENTERY AS IT OCCURS IN NICARAGUA.

By JUDSON DALAND, M. D.

[Read October 26, 1892.]

THREE varieties of dysentery are met with in Nicaragua, namely, the malarial, the endemic, and the epidemic, and of these the malarial is by far the most common. The prodromal symptoms of malarial dysentery are malaise, pain in the back, in the head, and in the umbilical region extending toward the pubes. In association with the diarrhœa these pains are highly characteristic of this form of dysentery. Mild cases are marked by very slight febrile and circulatory disturbances; whereas in the more severe cases, we have a moderate elevation in temperature, varying between 102° and 104° F. The stools are at first composed almost entirely of pure mucus, are small in quantity and are frequently attended by tenesmus; soon the mucus is streaked with blood. The pains are not usually severe during the act of defecation, but the pain in the head

and back is excruciating. Liver complications are not infrequent, particularly acute hepatitis or acute hepatic engorgement, each of which is frequently associated with jaundice. Hepatic abscess is a rare complication and is usually secondary to the ulceration of the colon. At times the spleen becomes greatly engorged.

Changes in the urine, indicative of kidney disease, probably exist, but chemical and microscopical examinations are rarely made from lack of proper instruments and reagents. Many of these cases of malarial dysentery are followed by intense anæmia and debility, lasting for several months.

When cases are seen early and promptly treated, the prognosis is almost uniformly favorable, but when seen late they usually die. As post-mortem examinations are never permitted, no information exists regarding the morbid anatomy or pathology of this interesting disease. The amœba coli, if searched for, would be found in many of these cases.

The treatment found most successful by Dr. Bermudez, of Managua, Nicaragua, is as follows: To an adult is given six grains of quinine morning and evening, in conjunction with—

℞.—Ammonii chloridi . . . . . gr. v.
Pulv. ipecac . . . . . gr. v.
Tr. opii . . . . . gtt. x-xv.
To be repeated every two hours.

The amount of laudanum is determined by the severity of the pain. When the pain is particularly severe and obstinate, morphine is superadded, and, in cases marked by debility, it is customary to substitute the carbonate for the chloride of ammonium in five-grain doses, every two hours, day and night.

In the way of food nothing is permitted except milk or milk and lime-water, to which sago may be added. The patient is allowed to drink freely of cool water, thus alleviating the intense thirst which is usually present. Ice water is considered harmful.

The *second* variety, known as endemic dysentery, resembles the preceding, but is very much milder, and is usually unattended by the fever or the severe pains in the head, back, extremities or abdomen that characterize the malarial form. The stools are composed of feces mixed with mucus and blood; are less frequent, and the tormina and tenesmus are less severe.

The average duration of malarial dysentery is three weeks, but occasionally it has been known to last two months, while very mild cases run their course in two weeks.

The treatment for this variety is the same as for the malarial, with the exception that the quinine is omitted. Almost all cases recover, and complications or sequelæ are uncommon.

The *third* variety recognized is called epidemic dysentery, which, as a rule, comes on suddenly, with pains in the head, back, throat, and extremities, accompanied with severe abdominal pains, shooting in character and centering at or about the umbilicus. Headache is particularly complained of; and not infrequently nausea and the vomiting of bile are associated. From the first the discharges are bloody, frequent, and there is intense pain and tenesmus. There may be as many as one hundred and fifty evacuations in the twenty-four hours, and an ordinary case may average twenty-four in the twenty-four hours, or one hourly, day and night. The temperature is high, ranging from 104° to 106° F., with a morning remission of two degrees, at which time there may be moderate perspiration. Severe cases die in less than seven days, and favorable cases may recover in from two to three weeks.

The discharges from the intestines continue bloody throughout the disease, but change in color, becoming dark and sometimes black from decomposed blood-pigment, and frequently they are vicid and tenacious, from admixture with mucus.

At times the patient becomes delirious, and occasionally coma supervenes. Children often develop twitching of the muscles, rolling of the eyes, and there is a tendency to bury the head in the pillow.

The complications usually noted are hepatitis, jaundice, and abscess of the liver. Usually so soon as hepatic complications occur the patient dies; in other cases epidemic dysentery is complicated by croupous pneumonia with rusty sputum, and it usually affects the base of the right lung. Now and then severe internal hemorrhages occur, and such an

accident explains the cause of sudden death which has been occasionally observed. In this form of dysentery the anæmia and debility are more marked than in the malarial form, and is more presistent. Not infrequently the patient suffers from obstinate constipation, due to stricture resulting from the healing of large and deep ulcers in the colon.

These cases are best treated by the administration of from ten to twenty grains of quinine given three times daily, and in addition chloride of ammonium, five grains; pulverized ipecac, five grains; and tincture of opium, ten to fifteen drops, repeated every two hours. Frequently, however, there is so much gastric irritability that these remedies are not retained, and in such cases the quinine is continued, but the chloride of ammonium and ipecac mixture is omitted, and fifteen grains of bismuth or five grains of tannic acid repeated every two hours, is substituted. When opium is indicated it is invariably administered in the form of the tincture, in doses of five to fifteen drops, repeated every two or three hours according to the severity of the case. At times nitrate of silver, in doses of one-sixth or one-eighth of a grain in pill form, is given every three hours. If the astringents mentioned prove of no avail, recourse is had to the acetate of lead, in doses of two or three grains every three hours. Most cases require stimulants, and experience has shown that alcohol in the form of brandy or whiskey is *inadvisable*, and that the best results are secured from the use of sherry, port, or any of the red or white wines, associated with the carbonate of ammonium, in ten-grain doses repeated every three hours.

The food is restricted to milk and lime-water, sago, farina. Not infrequently Dr. Bermudez has seen as many as one hundred cases in two months with the mortality of but 2 per cent., and his father would probably see as many as two hundred cases in the same length of time.

Dysentery is one of the most common diseases of Nicaragua, and typical examples of the disease may be seen any day in the year. Most cases of malarial dysentery are observed during December, January and February, while the epidemic variety occurs more frequently during the months of March, April and May. Of course, endemic dysentery is always present, and, as would be naturally expected, is equally prevalent at all seasons. The malarial form prevails chiefly in low, marshy districts, during the hot months. It is well to remember that the dry season, which corresponds to our Summer, begins in November and ends in April, the remaining months constituting the Nicaraguan Winter, or wet season. The average maximum temperature in the dry season is from 95 to 98 degrees. There is a difference of at least ten degrees between the temperature of the day and that of the evening.

The contagiousness of epidemic dysentery is fully recognized, and all ordinary precautions are taken to prevent the spread of the disease: Isolation, the free use of carbolic acid, the burial of all discharges, especially fecal and urinary; the burning of the linen soiled by the discharges; and in cases where the patient is too poor to submit to the destruction of clothing by burning, they are disinfected by boiling water.

In all these cases no researches have been made regarding the presence of the amœba coli.

Nicaragua has excited much interest of late, particularly in view of the probability that in the near future the Nicaraguan canal will become a reality, which will bring it into intimate relations with the entire world. I have, therefore, ventured to record these observations regarding a disease which prevails constantly, and at times becomes contagious.

My thanks are due to my friend and student, Dr. Salvador Bermudez, and to his father, who has practiced in Nicaragua for more than thirty-five years, for the description of dysentery as it appears in Nicaragua, and for the treatment which has given them the best results. The enormous experience of the physicians of Nicaragua has heretofore never been made known to the medical profession, in so much as they have no medical magazine to which they could report their observations; and, moreover, at no time has it been their custom to carefully note the cases under their care; so that this report is of particular value, and is,

perhaps, the first of the kind published in the English language. It is especially worthy of note, that the greatest confidence is placed in the use of the chloride of ammonium, and that this is their uniform practice. I would, therefore, suggest that it be employed in the United States, especially in the Southern States, where the climate more nearly resembles that of Nicaragua.

LECTURES ON THE TREATMENT OF HEPATIC AFFECTIONS.

By Dr. DUJARDIN BEAUMETZ,

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THE LIVER AS AN ORGAN OF SANGUIFICATION. (PHYSICAL CONSIDERATIONS.)

GENTLEMEN:—I am going to devote this conference to the study of the liver as an organ of sanguification. There is no more interesting study connected with hepatic physiology, and you will see how numerous are the practical applications deducible from the physiological considerations which I am about to set forth.

It is especially as an organ concerned with blood making that the ancients understood and spoke of the liver. Impressed by the special circulation of which the liver is the seat, and the large size of the blood vessels which enter and leave it, they regarded the liver as an abdominal heart, and Galen maintained that it was an organ in which were elaborated the most exquisite portions of the blood; he even added that it produced so large a quantity of blood that the alteration of its functions entailed the gravest maladies.

You will find, moreover, an allusion to this belief in the Talmud. In one of the twenty-one treatises of the Babylonish Talmud (the Bekhoroth) there occurs, in folio 55, this sentence of Rab Khahana, "The liver is a source of blood."

Not only did the ancients know the importance of the circulation in the liver, but they also knew the intimate relation between this circulation and hæmorrhoids, and they assigned to the latter an important role. They maintained that the sudden disappearance of hæmorrhoids produced grave disturbances and you will find many allusions to this belief in the writings of Hippocrates. Then came the period of decadence, when the liver was considered as a useless organ. But in the seventeenth century Stahl combated this opinion, and again attributed to the troubles of the portal circulation a considerable importance. It is in his celebrated thesis, *De vena portæ, porta malorum*, etc., etc., defended by his pupil, J. B. Gœthe, in 1698, that Stahl has set forth his views on this subject, and we see in our day one of our confreres defending this doctrine in affirming the pathogenetic role which chronic congestion of the liver plays in the genesis of diseases; I refer to the recent work of Poncel, of Marseilles.*

I desire to devote this lecture to the study of the physiology of the liver considered as a blood organ, and under two principal heads, I shall take up, first, the circulation of the blood in the hepatic gland, and then the modification which the blood undergoes in traversing this gland. You will see as we go on the importance of this study, based, as it must be, on the most recent discoveries of physiology, if we would have a clear understanding of the part which congestion of the liver plays in infectious and other diseases. Has this role been too much exaggerated? Has it, on the contrary, been too much overlooked? This is what we are now to ascertain.

To have a proper comprehension of the anatomical disposition of the hepatic circulation we shall have to review what has been taught on the subject and take up anew the study of the hepatic lobule.

You are aware that the conception of the hepatic lobule as it was understood by Hering and Kiernan has been profoundly modified these late years by the labors of Sabourin.

Hering took for the basis of his hepatic lobule, the blood vessels which it receives. Of polyhedral form, this lobule presents at its base the afferent veins which terminate in the hepatic veins, while the central vein of this polyhedron belongs to the portal vein; and within the vascular network established between the

*Poncel, *De l 'influence de la congestion chronique du foie dans la genise des maladies*, Paris, 1891.

veins of the periphery and the central veins, are placed the hepatic cells.

Sabourin, basing himself chiefly on anatomo-pathological investigations has abandoned the notion of hepatic lobules to adopt that of biliary lobules, and he has taken for basis of this system, the bile ducts, instead of the afferent or efferent veins.

The figure which I here present indicates very clearly the scheme of this theoretical conception of this new biliary lobule. At the base of this lobule (see figure 1) we find a bile duct (B), a terminal branch of the *vena portæ* (P), one of the terminal branches of the hepatic artery (A), while at the periphery of this biliary acinus are seen the branches of origin of the hepatic veins.

Starting from the periphery of the biliary acinus, the efferent kind find their terminus in the inferior vena cava; they traverse directly the liver, and are distinguished from the vena portæ by the fact that being adherent to the parenchyma of the liver, they remain gaping on section, while, on the contrary, the branches of the portal vein, protected by Glisson's capsule which envelops them, collapse after section. And, lastly, that the hepatic veins, like the portal veins, are deprived of valves. I do not need here to describe the origin of the vena portæ. You all know that three large veins contribute to form it; the superior and inferior mesenteric and the splenic, and it is the reunion of these three veins near the head of the pancreas which constitutes the trunk of the portal vein which enters the liver by the transverse fissure. All the blood then coming from the surface of the intestinal mucosa passes through the liver by the portal circulation. At the same time, you must here note that there exist accessory portal veins which Sappey described in 1859, and which are divided into five groups, to which he gave the names of *gastro hepatic group, cystic group, group of nutrient veinules, group of the suspensory ligament*, and lastly *umbilical* or *para-umbilical group*.

To this portal circulation, which constitutes the dominant fact in the physiology of the liver as a blood organ, you must add the nutrient vessels furnished by the hepatic artery.

Now that you understand the dispotion of the hepatic circulation, let us see how this circulation is carried on. The absence of valves in the network of the vena portæ, the absence of surrounding muscular masses aiding by their contraction the movement of the blood, the presence of a capillary network which the blood has to traverse in the interior of the liver, and lastly the vertical disposition of the entire vascular network, are so many circumstances which oppose the passage of the blood from the intestine to the inferior vena cava.

But to these causes of retardation of the circulation, we must oppose those which favor the course of the blood. That which is superior to all others is assuredly respiration, which acts in two ways In the act of respiration the diaphragm falls, it compresses the entire abdominal mass and forces the blood from the abdomen toward the vena cava.

On the other hand, by the respiratory movements a vacuum is created in the thorax, and the venous blood is, as it were, asperated into the right auricle.

Rosapelly who, in 1873, wrote an able thesis on the "Cause and Mechanism of the Circulation in the Liver," a thesis well worthy of being consulted for the full information which it gives as to all the details of the hepatic circulation, has made strikingly prominent this effect of the respiratory act. In fact, when in an animal you examine the blood pressure in the vessels which enter and in those which leave the liver, this is what you notice: the pressure in the vena portæ just before it enters the liver oscillates in the day between 7 and 20 millimetres of the murcurial column, while, on the other hand, in the veins that leave the liver, this blood pressure is only 3 to 4 millimetres, and even this is, so to speak, negative, and is ordinarily from 7 to 8 millimetres.

Inspiration is, as you see, one of the great factors of the hepatic circulation, hence everything which disturbs the respiratory movements, and in particular, every impediment to inspiration affects unfavorably the hepatic circulation and retards it, and we shall see what consequences are deducible from this fact when we come to examine the pathogeny of hepatic congestions and their treatment.

Besides this marked effect of respiration, and in particular of inspiration, on the hepatic circulation, we must also add that this circulation is aided by real contractions of the walls of the portal vein. The trunk of this vein possesses a muscular coat sufficiently developed to produce rhythmical beatings of the vein.

Rosapelly has also studied the quickness of the circulation in the postal network, by making use of the yellow prussiate of potash. On introducing one gramme of this substance into the portal vein, he finds this salt in the hepatic veins in eight seconds after its introduction. It is at end of from twenty-five to thirty seconds that he finds the maximum quantity, and in one minute there is no longer a trace of this salt in the hepatic veins. Here, also, the influence of respiration makes itself felt, and anything which impedes the breathing of the animal under experimentation greatly retards the appearance of the prussiate of potash.

In basing himself on experiments made, not on the liver of the living animal, but on the dead liver, and in making use of Ludwig's method of artificial circulations, a method of which I gave a description in my first lecture on the antiseptic liver, Rosapelly has arrived at this conclusion: In the portal vein the rate of the circulation is 33 millimetres a second; in the bifurcations of this vein, it is only 22 millimetres. The rate becomes much less in the capillary network, being only from 4 to 5 millimetres, while it is 16 millimetres in the hepatic vein.

Rosapelly has also studied the causes of obstruction to the circulation; he has demonstrated, first, this fact, that when the pressure of the blood in the hepatic vein is equal to that of the portal vein, the circulation is arrested. As for the arterial circulation, it stops as soon as the pressure in the hepatic veins rises, although still remaining very much less inferior than that of the portal vein. Hence he adduces this fact, that the circulation is much more active in the portal vein than in the hepatic artery, and it only requires a slight impediment to the circulation in the superior cava and in the auricle to rapidly entail modifications in the arterial circulation, *i. e.*, in the nutrient blood-flow which goes to the liver.

As for the quantity of blood which passes through the portal vein in twenty-four hours, it is considerable, for in a dog weighing 20 kilogrammes, Slogga found a blood-flow of 500 grammes per minute, which makes almost 721 kilogrammes of blood passing through the liver in twenty-four hours. Moreover, as Monneret has shown the liver may increase to triple its volume and weight under the influence of blood stasis. Lastly, you should not forget that the nervous system has a great influence on the hepatic circulation, which may be profoundly modified by lesions of the cord, of the sympathetic, and even of the pneumogastric.

I come now to the second part of this lecture, *i. e.*, to the modifications that the blood undergoes in its passage through the hepatic gland. Considered in their aggregate, these modifications pertain to the water, albumen and fats, as the following table shows:

	Water, per c'nt.	Album, per c't.	Fat, per ct.
Blood of the portal vein,	76.921	24.459	3.225
Blood of the hepatic vein,	68.646	16.763	1.686

It is a diminution in the proportion of water, of albumen, and of fat. The hepatic cell, then, must retain or modify the blood coming from the intestines, and if we consider that the vascular net-work absorbs from the surface of the intestinal mucosa, water, peptones and perhaps a certain quantity of fatty matters, we easily understand the important office of the liver which thus takes from the blood a part of its water, albuminoid substances and fats, which have found their way there from the alimentary canal.

How is this modification effected? Does a veritable combustion take place? This leads me to allude to one of the most delicate points connected with the liver as a blood organ. I refer to the liver as a producer of urea.

We may as well first settle the first question. The liver is, as Galen taught, an organ that generates heat, and this is sufficiently intelligible when we think of the numerous chemical processes taking place in the hepatic parenchyma.

When, in fact, we examine the temperature of the blood in the portal vein and in the hepatic veins, we notice that

this temperature is represented in the the following ratio:

Blood of the portal vein, ... 40.°2.
Blood of the hepatic vein, ... 40.°6.

As you see, the blood of the hepatic veins has a higher temperature than that of the portal vein. But I repeat, this is a secondary consideration, for it is easy to understand that the glycogenic function and even the secretion of bile may be the cause of this elevation; and I come now to the much more disputed question of the liver considered as a producer of urea.

Scarcely had urea been discovered in the urine by the younger Rouelle in 1772, than Fourcroix and Vanquelin affirmed that the variations in the production of urea were dependent on diseases of the liver; and the experiments of Heynsius, of Stokvis, of Fürher, of Ludwig, of Meissner and of Cyon showed in multiple ways that the liver contained urea, and that the blood which traverses this gland becomes charged with this principle.

Murchison in 1874 summed up the investigations, and also concluded that urea exists in considerable quantity in the liver and is formed there.

Lastly, in an article which attracted great attention, Brouardel in 1875 came to the following conclusion, viz., that the quantity of urea secreted in twenty-four hours is dependent on two principle influences: 1, the integrity or alteration of the hepatic cells; 2, the greater or less activity of the hepatic circulation.*

Nevertheless, this view was not admitted by all, and a great number of physiologists were disposed to adopt the view expressed by Dumas, who taught that urea resulted from the oxidation of azotized matters in the entire economy, an opinion based on the celebrated experiment of Bechamp, who showed that in oxidizing albumenoid matters with permanganate of potash we obtain urea, and it is admitted that there is always a direct relation between nutrition and the production of urea.

Hence, the opponents of the theory that the liver is exclusively a producer of urea took the ground that if in diseases of that organ we observe a diminution in the excretion of urea, it is because they entail by themselves a profound disturbance in the general nutrition.

Among the opponents of the exclusive doctrine we must also claim those who taught that the kidney fabricates urea, regarding the renal parenchyma as a veritable gland; and this view was chiefly maintained by Hoppe and Seyler, Hopples and Talesky. Nevertheless, despite these reservations, if we may trust the more recent experiments of Slosse it seems experimentally demonstrated that when you destroy the hepatic cells by the procedure which consists in ligating the intestinal arteries (an operation which entails the death of the hepatic cells*) the excretion of urea disappears.

It seems then to-day demonstrated that if the organic combustions may fabricate urea in all points of the economy, it is, nevertheless, to the liver that the most important role in urea-production belongs.

Physiologists have even endeavored to carry the inquiry further, and to ascertain from what elements the liver fabricates urea, and some have supposed that it is with ammonia and an azotized radical that urea is thus constituted. They have appealed for support of this opinion to the experiments undertaken by Minkowski, Naunyn and Stern, who extirpated the liver in animals and detected ammonia in the blood and excretions. Nevertheless the fact does not seem to be absolutely demonstrated, and the results appear to be different, according to the animal under experimentation.

Neubelthan, in vivisections on frogs arrived at results almost similar, for in examining the urine of frogs from which he had removed the liver, he found ammonia.

This is not the only function affecting the blood which the liver exercises. It remains for me to speak now of its hæmatopoietic functions.

You know, as I have already told, that the liver was considered by the ancients as a blood-making organ, and you will see that recent experiments tend to justify

*Brouardel, Urea and the Liver (*Archives de physiologic*, 1875, p. 373-551.)

*Slosse. Does the liver form urea? (*Jour. de Medicine de Bruxelles*, July 20, 1891, p. 417).

Galen's hypothesis. Only, the opinions which have been put forth on this subject are absolutely contradictory, some affirming that the liver forms blood corpuscles, others that it destroys them.

Lehmann, defended the first of these views, and by relying on experiments made on dogs and horses, he always found in the hepatic vein more corpuscles than in the portal vein. Below is Lehmann's table :

In 1000 parts of the blood of a horse.	Portal vein.			Hepatic vein.		
	I	II	III	I	II	III
Globules,	601	573	257	776	745	573
Plasma,	399	427	743	224	259	427
In 1000 parts of the blood of a dog.						
	I	II	III	I	II	III
Globules,	460	447	450	675	650	748
Plasma,	540	553	550	305	350	252

This view has been combated by Schultz and Mandl, who have affirmed that the liver destroys the red globules through the action of the choleates, and that as a consequence of this destructive action, we have hæmaphein and perhaps bilirubin.

But since the processes of enumeration of the globules have been perfected, there seems reason to believe that the liver has little action on the blood, and if there is any at all, it is rather destructive than productive of red globules.

These, moreover, are Hayem's figures, showing the difference which exists from the point of view of the number of globules between the blood of the portal vein and that of the hepatic veins:

BLOOD OF THE PORTAL VEIN.

Red Globules,	7,773,000
Hæmatoblastes,	238,000
White Globules,	6,350

BLOOD OF THE HEPATIC VEIN.

Red Globules,	7,700,000
Hæmatoblastes,	228,000
White Globules,	7,900

As you see by this analysis, the blood of the portal vein is richer in globules than the blood of the hepatic veins, but the difference is very slight.

To finish what pertains to the role of the liver in nutrition, I will say a few words about the action of the liver in the production of fat.

Many physiologists have maintained that adipose tissue is not formed directly from fats in the ingesta, but is made in the liver, out of hydro-carbon and even albuminoids. In some experiments made on animals, and in particular on geese, Boussingault, after Liebig, showed that the quantity of fat accumulated in the organism of these animals much exceeded that contained in the corn on which these animals were exclusively fed.

Persoz stated the view more clearly, and maintained that in geese, and probably in other animals, the liver had for its function, and as far as adipose formation is concerned, the exclusive function to transpose starchy matters and sugar into fat.

This view is somewhat exaggerated; it is probable that the entire economy participates in this transformation, and when speaking of Diet in Obesity, I took up this subject of the formation of fat and showed that obese persons transform starch and glucose into fat to a much greater extent than they assimilate fatty substances contained in their food.

Having finished what I had to say about the properties and offices of the liver, I shall in the two following lectures speak of the pathological deductions which flow from these facts. The first shall be devoted to hepatic congestions, the second to cirrhosis. In a final lecture I shall take up the treatment of hydatid cysts.

Annotation.

PARASITISM.

ONE of the most important sociological inferences may be drawn from the demonstrated effects of a free, relatively highly organized animal descending to parasitism. As soon as it is habituated to drawing its sustenance from its host it becomes a veritable living gut, eating, drinking and excreting, losing its brains, eyes, ears, respiratory and locomotory organs, as useless in its new state.

For identical reasons, many offspring of wealthy parents undergo comparable degradation into simpering, brainless, worse than effeminate chatterers. When the rich consent to being educated, they will see the need of their children having some useful occupation, not one to increase their money accumulation, but one

that will develop intellect and avoid their earning the vulgar but apt title of "gut," or parasite, fop, dude, or other equivalent.

It would not be believed by the cane-sucking, monocled "sassiety-man" of wealth, that the shoemaker, blacksmith, and even the street-sweeper are his intellectual superiors. What they do has some useful end, however humble. The human parasite has no need for any organs but entrails, and a few generations of him develop his descendants figuratively and morphologically in that direction.

S. V. CLEVENGER.

HABIT CHOREA.

WIER MITCHELL aptly distinguished as "Habit Chorea" the facial grimacing often seen in adults and frequently improperly diagnosed as ordinary chorea.

Recent experience leads me to look for astigmatism and myopia as causes, the cause of the trouble not being recognized by patient or relatives, and the facial contortions are but ineffectual reflex attempts toward better vision. Nor need this statement that astigmatism and myopia is an occasional cause of the "habit chorea" facial spasms be distorted into a "Steven's-fiasco" admission that pregnancy, epilepsy, all insanities, kidney disease, hæmorrhoids, quinsy, inebriety, flatulence, constipation, old age, etc., are caused by ocular troubles to be remedied by snipping of recti muscles.

In Chicago we have a rectal fiend (homœopath) who, for a consideration, will cure everything by paralyzing the sphincter ani. Every train brings him patients, and he makes $25,000 yearly where honest, intelligent physicians starve.

S. V. CLEVENGER

70 STATE STREET.

VACCINATION PAYS.

WE do not always realize the blessings we have in the immunity from escapable evils. The following statement should make us grateful for vaccination and encourage the fight against anti-vaccinationists. During the past season small-pox has invaded the ancient city of Van, the Armenian capital of Asiatic Turkey. It is a place where there is no vaccination among the native population, and where pianos are brought in from Surria by men lifting by means of poles attached to the boxes. A lady saw in a ride through the city five cases of small-pox playing in the streets. Certainly panic did not kill the two thousand persons who died of small-pox in this epidemic. *Cases were not reported!*

Population 35,000. 57 cases to the 1,000. Taking the population of New York at 1,700,000 this mortality would be about 96,597 deaths in one season. Most of the two thousand deaths were children. Adults escape simply as they have had it and got well. During this epidemic Van was quarantined against cholera, and not a case occurred in the city, and yet there was no vaccination, when there was one death to every 17.5 population.

What lessons may we learn from these ethics of Van. Certainly one lesson is to acknowledge our debt of gratitude to vaccination and be vigilant in having the public avail themselves of it continuously.

E. C.

DREARY READING.

THE dreariest reading that comes to our exchange table, is to be found in the alleged "Health" and "Temperance" publications. Why does not some enterprising citizen cut the ancient saws, the chestnutty anecdotes, the endless repetitions of good advice that nobody wants and moral platitudes that nobody reads, and substitute something interesting? Something that is alive; useful; modern; real; practical; something tangible to work on, to talk about, to read to one's neighbor, and argue and get excited over; something that will arouse the stagnating nervous currents and quicken the intellect. We have a kindly regard for the man who tried to start a crusade against bread, as being a rank poison; for, in the effort to reply, some persons learned more of physiological chemistry than they ever knew before. We would suggest as a new topic, the advocacy of public urinals, as a means of promoting temperance and hygiene at the same time. This is one of the obviously needed reforms, that only require public agitation to render them successful.

The Times and Register.

A Weekly Journal for Medicine and Surgery.

WILLIAM F. WAUGH, A. M., M. D.,
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TREATMENT OF SYPHILIS.

THE following sketch has been written in response to repeated requests for a detailed exposition of the treatment of syphilis and of the principles on which it is based. Although the matter is one of the simplest and best defined in the whole range of medicine, we continually see physicians following false lights and slipping up in their practice for want of attention to the plain facts of the case. The present position of the best modern syphilographers may be briefly summed up as follows:

1. The special cause of syphilis is to be found in the tissues whose growth is due to this cause; the syphilitic tissues.

2. These tissues have less vitality than the normal tissues of the body, and tend to spontaneous destruction, breaking down into ulcers readily.

3. Mercury, given in sufficient doses, tends to produce destructive changes in both healthy and diseased tissues.

4. As the syphilitic tissues are of lower vitality than the healthy tissues, the destructive process can be produced in the former by a smaller dose of mercury than in the latter.

5. Experiment will show how large a daily dose of mercury can be borne by each case without causing salivation; and this daily dose can be given for an unlimited period.

6. If at any time symptoms of salivation manifest themselves, the dose must be lessened, slightly, but not omitted altogether; while if no such symptoms are presented after some weeks' medication, the dose should be gradually increased. The object is to obtain the greatest possible effect on the syphilitic tissues without effecting the healthy tissues injuriously.

7. The treatment should be continued for several months after every *obvious* indication of syphilis has disappeared.

In practice, each one prefers the form of mercury, with which he is most familiar. I have employed all the known forms, and prefer the most active preparations, because they *are* most active, and most easily regulated. In all cases it must be remembered that when a syphilitic neoplasm has been formed, it has destroyed some normal tissues that will never be reproduced. When a gumma appears in the brain, some of the nerve fibers are broken, some compressed. Remove the new growth and the compressed fibers resume their functions; but it is very problematic if the broken ends ever unite. For this reason it is a good rule to go to work with briskness, and to bring your syphilitic patient rapidly under the influence of the mercurial. In reality there *is* an action on the normal tissues as well as on the diseased. While a patient is taking an active mercurial course the vital processes are all accelerated; waste is increased enormously, and old accumulations of water, fat and detritus are carried out of the body. Patients have the same

curious sensation of regeneration that is experienced during convalescence from septic fevers; only that during the mercurial course there is a real increase in the vital forces, whereas in convalescence it is only apparent.

Suppose we choose the mercurial pill as our best agent. A man will take from twenty to thirty grains daily, with good effect. We begin with ten grains and add two each day. When we have reached twenty grains the patient's gums begin to show signs of approaching ptyalism, sponginess, tenderness of the teeth on biting hard substances, etc. Then we drop to eighteen grains daily, and continue. In a few days the signs of ptyalism are still present, and we drop to sixteen grains. Then the signs disappear, and if they do not return in two weeks, we begin to creep back to the larger dose. If, however, we find that, whenever we touch twenty grains a day, the gums become tender, we settle down to eighteen grains and adhere to that dose, unless forced to lower it.

I prefer to give the blue mass alone, with no opium, preferring to let it run off by the bowels, the kidneys and the skin. The free elimination is beneficial, as is shown by the success of the Hot Springs treatment, and by the moderate usefulness of vegetable eliminants. In all cases, at all ages, where mercury can be employed at all, the blue pill answers every useful purpose, and no other mercurial is required.

But scrofulous persons cannot take mercury very well. The tendency to spontaneous degeneration of tissues, with suppuration or ulceration, is sufficient in some of these cases to render mercury inadmissible. If the blue pill cannot be used, the astringent salts may be employed, but are generally quite as harmful. It is better to build up the patient with good food, quinine, cod-liver oil, and especially the iodide of iron; the iodides of sodium and potassium also are of value, as they seem to keep the disease in check until the patient is strong enough to take mercury. Some can never reach the mercury point, and for them the iodides must be permanently substituted. These remedies require much longer periods; if, indeed, they ever thoroughly eradicate syphilis from the system, which I doubt. However, if scrofulous people contract syphilis, they must be content to take iodides the rest of their lives, if need be.

Gold and platinum have properties similar to those of mercury, but have no advantages to make amends for their higher price. The same rules govern their administration.

Congenital and infantile syphilis require the same treatment as the other forms; the same preparation, and the same method of ascertaining the largest dose that can be given with impunity.

When it is desired to act quickly, as in cases of cerebral syphilis, the mercury may be given by inunction, hypodermic injection and fumigation, and the iodides added, in heroic doses. The object is here to check the syphilitic process as speedily as possible, at all hazards, before irreparable damage has been done to the delicate cerebral tissues. Ptyalism is a small danger in comparison, and the risk of its occurrence must be accepted, while the system is brought under the influence of mercury as rapidly as possible.

The hypodermic method must be still held as *sub judice*. Its advocates claim too much. Calomel is nothing but calomel, however administered; and that a grain or two injected under the skin once or twice a month exerts more effect than the regular administration during this period by the mouth, sounds like the assertion of an enthusiast rather than the sober judgment of a conservative man.

In all cases where there are accessible

litic lesions, mercury should be employed locally as well as constitutionally. The union of these two methods of administration accomplishes much more than either is capable of doing singly. The biniodide is my own favorite for local use; in solution or ointment. For the latter I prefer lanoline as a base.

In conclusion I would say that the best success will be obtained when this method is followed out neither rashly nor timidly; with judgment and persistence. If we give the powerful agent recommended, rashly and heedlessly, a salivation will occur of such gravity as to compel the discontinuance of mercury, and the disease will at once be reproduced, while our patient's confidence has been shaken. If the drug be given too timidly, the disease will keep abreast or ahead of the remedy, and the patient will become disgusted with its inefficiency, and go to some one else.

W. F. WAUGH.

ELIMINATION OF TOXIC AGENTS BY FREE PERSPIRATION.

IN *Science* W. H. Wooster describes the case of a little girl, bitten by some unknown venomous creature, and presenting symptoms betokening impending dissolution. The child was placed in a tub of hot water, enveloped to the neck in blankets, to induce sweating. A hot-pack followed, with stimulants internally. Within fifteen minutes the threatening symptoms began to subside, and within an hour the child was well.

The elimination of toxic substances by means of profuse sweating is not a new, but rather a neglected, idea. Cases of suppositious hydrophobia have been reported, in which recovery followed the action of a full dose of jaborandi. The same agent has been given with asserted success for the toxic symptoms following bites of venomous serpents, scorpions and spiders.

The writer has been informed, by the proprietor of the leading Turkish bath establishment in Philadelphia, that several such cases have been treated successfully by these baths. It is well known that ordinary alcoholism is speedily dissipated by the Turkish bath. A man may be brought in, in alcoholic coma, "paralyzed drunk," and within an hour he walks out with head erect, perfectly sober. Such cases are numbered by hundreds and thousands at the establishment referred to. The same success has been obtained in the treatment of several cases of opium poisoning; while the writer can testify to the great value of the Turkish bath as an adjuvant in the treatment of the alcohol and of the opium habits. It is also the surest and safest means of aborting an acute catarrh, of the nose, larynx or bronchi; while we have been informed by credible witnesses that it is equally effectual in the earliest stage of specific urethritis. No more effectual remedy can be found when uremic symptoms supervene in the course of a chronic nephritis. Indeed, in all forms of Bright's disease, the Turkish bath is of the utmost value.

When headache, rheumatoid pains, indigestion, etc., indicate the presence of the uric acid diathesis, the best of all eliminants is this bath. In true gout, it takes the place of active bodily exercise when the latter cannot, or will not, be taken. Whenever the blood is charged with the products of imperfect digestion, or of waste, the speediest, pleasantest and most effectual means of elimination is to be found in this system of manipulations and sweating.

As to influenza, opinions appear to differ; although it is not easy to find anyone who claims that the Turkish bath does harm, while hundreds are enthusiastic over it as a means of prevention or of cure.

This subject might be pursued at much

greater length; but enough has been said to show that in the Turkish bath we have an agent of remarkable power as an eliminant, and whose therapeutical applications are only beginning to be understood by the profession.

Letters to the Editor.

ECZEMA WITH ANGINA PECTORIS.

I AM an old broken down physician who has for four years suffered from eczema of the right leg. It extends from the thigh to the ankle. It began as a popular eruption on the outer aspect of the leg, and extended over my whole body within several months. The eruptions then subsided in part, but remained permanently fixed in the locality first attacked. For over four years I have suffered with almost unbearable burning, itching and pain. The affected part swells and aches. It seemed to be an eczema of the squamous variety; the skin was dry, and when scratched appeared white and branny. At present it is too tender for scratching, and is disposed to inflammation and suppuration.

At the time the eruption receded, I was seized with symptoms resembling angina pectoris, *i. e.*, a choking sensation at the neck and in the cardiac region, with pain extending to the left shoulder and down that arm to the hand. This gives me no trouble at present, except after sudden exertion or when I am slightly fatigued, when I feel it slightly.

My general health is good; I am of full habit, have good appetite, am 68 years old, and weigh 168 lbs. My teeth are so bad as to hinder the thorough mastication of my food. I use a good deal of tobacco. I would like to have your opinion of my case and your suggestions as to its treatment.

G. W. STORY, M. D.

[The eczema at present requires soothing applications, such as bismuth subnitrate in fluid petrolatum—one part to ten. This should be applied until the irritation has been subdued, and then an ointment of ammoniated mercury, in lanoline, one part to twenty, substituted. Internally, the sulphide of arsenic should be given, in doses of gr. $\frac{1}{134}$, three times daily, gradually increased until the effects of the arsenic begin to appear. Brucine may be added as a heart tonic, gr. $\frac{1}{67}$, thrice daily, increased to gr. $\frac{1}{12}$. These remedies should be continued for a long time, unless they disagree, when others should be substituted; such as tar ointment locally, and the compound syrup of hypophosphites internally. The digestion should be carefully regulated, and all food forbidden that has shown any tendency to aggravate the irritation. All washing of the leg is to be forbidden; the ointment once applied is not to be removed unless to make way for another, when a change is decided upon. If the state of the heart permits, an occasional dose of pilocarpine will be of much value; enough to cause some sweating, but not too much.—W. F. WAUGH.]

QUERIES.

I have just received the Gonorrhœa Number of THE TIMES AND REGISTER and want a copy for a young friend. Please send me one for which I enclose ten cents. Please give me your ℞ for the bismuth and fluid petrolatum mixture for injection and how often to use it. I did not know that there is fluid petrolatum. If you use oil as the menstruum, what kind of oil do you use?

Do you use the bismuth sub-iodide of the same strength as the sub-nitrate? How is boric acid for an injection, two or three grains to ounce of mucilage?

Which is the better work to refresh my old and failing mind, Professor Osler's of John Hopkins, Baltimore, or a recent treatise on the Practice of Medicine by Professor Lyman (I think) of Chicago, I want a compact practical work which gives the best treatment at the present time with the best selected remedy. Your special numbers are gems of the purest rays and greatly enhance the practical value of your journal. Glad to learn of your addendum on Typhoid Fever. May you prosper greatly in your valuable labors to advance the best treatment of diseases.

G. H. A.

[The sub-nitrate of bismuth or sub-iodide may be employed in the same doses. Either may be mixed with fluid petrolatum, or fluid cosmoline, in the proportion of one part to ten, and used as an injection without any other ingredient. This oil is the least likely to be irritating from rancidity, and in itself exerts a beneficial action on inflamed surfaces.

Boric acid is a feeble agent, and in the strength of five grains to the ounce of mucilage will not do much harm—or good.

Osler's new work on Practice of Medicine is fully deserving of your confidence. The path-

ology is fully up to date, and given in admirably distinct and simple diction. The treatment is conservative, and trustworthy, as far as it goes. I have not seen the Chicago work you mention, but anything of Lyman's should be good.—W. F. WAUGH.]

CHOLERA IN ANTWERP.

IN conversation with one of the physicians of this city, I requested him to give me a little sketch of his personal experience with the cholera, which existed in a sporadic condition in Antwerp the last Summer.

All his cases, he said, existed among the boatmem belonging to the barges which transport freight to the interior of Belgium and France through the Canals.

He attributed the existence of the disease to the fact that the bargemen drank the water from the canals, as proof of which, after the authorities supplied them with city water, he had but three cases in one week, whereas the week previous he had fifteen.

The patients were treated on the boats, provided there was any accommodation; otherwise they were taken to the hospital, when a special building had been set aside. The boats and occupants were isolated the effects of patients disinfected, as well as clothing and persons, and in no case did the disease spread beyond the person affected.

The treatment in all cases was about the same. Salol, beta naphthol, bismuth, camphor, ether and the milder opium preparations. Ice and citric acid drinks in large quantities, warm sand bags and hot water to the abdomen. Intravenous injections of saline fluids were not tried with his private or hospital service, being considered too dangerous, besides they were not supplied with the necessary apparatus. This method was employed in several universities in Belgium with but slight success. Two of his cases died without vomiting or passing a stool.

One, a woman seven months pregnant, died in two hours. The other, four months pregnant, died in three hours. He treated in all thirty-seven cases, of which thirty-five died; a boy thirteen and a man thirty-five being the only survivors. The general mortality was not so bad in this city, however, being I think, under fifty per cent.

Cholera was never epidemic in this city during the past summer. There was no connection between cases, all being isolated ones. The smaller tradesmen and hotel proprietors suffered from the absence of tourists, who were deterred from coming here on account of the exaggerated reports. The general business of the city and port was not affected, and one living here would not have been aware that the plague existed within the city.

T. C. SANGREE, M. D.

ANTWERP, BELNIUM.

News and Miscellany.

Deaths and Interments in the City of Philadelphia from the 5th to the 12th of November, 1892.

CAUSES OF DEATH.	Adults.	Minors.	CAUSES OF DEATH.	Adults.	Minors.
Abscess	1		Hemorrhage		1
Aneurism of the Aorta.	3		Hernia		1
Alcoholism	2		Homicide	2	
Apoplexy.	16		Inanition		3
Asthma	2		Inflam'n Bladder	2	
Anæmia	2		" Brain	2	2
Bright's Disease	14	1	" Bronchi	8	3
Burns and Scalds	2	2	" Kidneys	7	
Cancer	9		" Larynx		1
Casualties	8	2	" Lungs	28	13
Congestion of the Brain	1	1	" Heart		2
" Lungs	1	2	" Periton'm	7	
Cholera Infantum		5	" Pleura	1	
Cirrhosis of the Liver	2		" S. & B'wls	1	4
Collapse of Lungs		1	" Parotid Gland		1
Consumption of Lungs	46	6	" Spine	1	1
Convulsions		9	Marasmus		14
Croup		1	Neuralgia of Heart	1	1
Cyanosis		5	Obst'n of the Bowels		3
Debility	4	1	Old Age	8	
Diabetes	3		Purpura Hemorrhagica		1
Diarrhœa	2	1	Paralysis	4	1
Diphtheria		49	Poisoning	1	
Disease of the Heart	24	2	Rheumatism		1
" Liver	1		Surgical operation	3	
Drowned	1		Suffocation		2
Dropsy		3	Suicide	3	
Dysentery	2		Teething		1
Epilepsy	1	1	Tetanus	1	1
Fatty Degeneration of the Heart	2		Tumor	2	1
Fever, Intermittent		1	Ulceration of the Stomach	1	
" Malarial		1	Uræmia	5	
" Scarlet		9	Whooping Cough		2
" Typhoid	2	1			
Goitre	1		Total	243	161

Francis Murphy is conducting meetings in Cincinnati to boom Keeley. The *Lancet Clinic* says; "He ascribes 'salvation from drink to the grace of God *and the wonderful Keeley cure*.' Our impression is that if the former is inefficient, there is no use of trying the latter; and we decidedly object to the two being coupled together in the above sacriligious manner. If there is anything on this footstool that the grace of God is not sufficient for, it must be the methods adopted by these institutions."